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XXVIII. *A Supplement to Mons. Pingré's
Memoir on the Parallax of the Sun: In a
Letter from him to the Royal Society,
Translated by M. Maty, M. D. F. R. S.*

Gentlemen,

Read May 17, 1764. **I** Had the honour to send you my memoir on the Parallax of the Sun as deduced from the observations of Venus; give me leave to add to it some remarks which I have since made, and communicated to our academy. The learned Societies of Europe, amongst which yours holds a most distinguished rank, will be the judges of the success of our expeditions. A definitive decision will probably not be formed till after the observation of the transit of 1769; in expectation of further accounts, which may tend towards this decision, there is at least one which I now submit to your examination.

Supplement to my Memoir on the Parallax
of the Sun.

I impatiently expected the first volume of the Philosophical Transactions of the year 1761, where I hoped to find some observations that might determine which of the observations, *viz.* that of Mess. Mason and Dixon made at the Cape of Good-Hope, or that which was made at the island Rodriguez by Mr. Thuillier and myself, deserved the preference. The first reduces the Sun's Parallax to $8'' \frac{1}{2}$ at most, whereas

whereas the latter increases it to near $10'' \frac{1}{2}$: the difference is too considerable not to deserve an enquiry into its causes. The expected volume is at last come to hand; and my first care has been to examine with the most scrupulous attention the observation made at the Cape. I owe this testimony to truth, that this observation, as well as all the others of Mess. Mason and Dixon, appears to me to have been made with great judgment. An exception might however be made as to the extensive description of an appulse of σ Sagittarii to the Moon's southern limb, which is found page 389. This certainly was more than an appulse, and the star was really immersed at the Cape.

In this same volume, I found two observations, which would be decisive, if time and other circumstances had permitted them to be made with sufficient accuracy. I have very carefully calculated them both. Mr. Maskeline observed at the island of St. Helena, situated at $15^{\circ} 55'$ South latitude, and according to Dr. Halley at $33' 17''$ of time West of the Observatory at Paris; but this determination of the longitude does not seem sufficiently exact. I have compared many observations of Jupiter's Satellite's immersions and emersions made at the island of St. Helena by Mr. Maskeline with the corresponding ones made at Paris at the Marine Observatory by Mr. Messier, and have only found $31' 56''$ for the difference of longitude between the two places; and as the Marine Observatory is $2''$ East of the Royal Observatory, I think I may conclude that the place where Mr. Maskeline observed is only at $31' 54''$ West of the Royal Observatory.

The clouds greatly obstructed Mr. Maskeline's observations; he could make but one that was useful. At $7^h 31' 07''$ in the morning, apparent time, the bodies both of the Sun and of Venus being perfectly well defined, the distance of the nearest limbs was, by means of an object-glass Micrometer adapted to a reflecting telescope according to Mr. Dollond's invention, found to be $1' 44'' \frac{3}{4}$. I have computed that by allowing $10''$ for the horizontal parallax of the Sun, from that phasis to the internal contact of the limbs, there must have passed $34' 52''$ at St. Helena; I have even assured myself, by some other calculations, that, by an alteration of one or two seconds in the parallax, and of several minutes of time in the longitude of St. Helena, the interval which I have just now determined would suffer an increase or diminution but of very few seconds. The internal contact of the limbs must therefore have happened at St. Helena at $8^h 05' 59''$ in the morning. One can't suppose it to have happened later, because $17'$ after, or at about $8^h 23'$, the skies having cleared again, nothing more could be seen, and the external contact was over. This observation being compared with that of Tobolsk, would give $11''$ for the horizontal parallax, which is a little too much. Mr. Maskeline observes, that tho' Venus's limb and the Sun's appeared as defined as could be desired, yet when the artificial internal contact of Venus's limb with the Sun's was made by means of the object-glass micrometer, Venus's limb dilated and contracted itself alternately, getting and losing a small space within the Sun's limb. He adds, that he endeavoured to take it in the middle of this vibration, but dares not affirm that he exactly

actly did so. Let the distance determined by Mr. Maskeline be only diminished by $2''$, and his observation will perfectly agree with mine; but in order to make it tally with that of the Cape, it would be necessary to diminish this distance by 10 or $11''$, and it is not very likely that Mr. Maskeline should have committed such an error. His observation might likewise be brought to coincide with mine, by making a diminution of 40 or $45''$ in the Western longitude of St. Helena, as I just now settled it; whereas it would be necessary to take several minutes from that longitude in order to make the observation agree with that of the Cape, which does not seem possible. Hence, though I don't take Mr. Maskeline's observation to be in itself absolutely decisive, yet I am persuaded that it adds great weight to the exactness of mine.

Fort St. George at Madras is, according to Mr. Hirst, $13^{\circ} 8'$ North latitude and $3^{\circ} 4''$ of time Eastward of Pondicherry, and consequently at least $5^{\text{h}} 12' 54''$ Eastward of our Observatory's meridian. Mr. Hirst's observation is related in the Philosophical Transactions; the interval observed by him between the two internal contacts was $5^{\text{h}} 51' 43''$, greater by $2' 49'' \frac{1}{4}$ than that which was observed at Tobolsk. This would give $9'' 56$ for the Sun's horizontal parallax, a quantity which is about a medium between the Cape observation and mine. The comparison between the time of observing the contacts, and that of the corresponding observations made in other places, gives conclusions so vastly different, that I dare not relate them here. Mr. Hirst gives a sufficient account of his instruments, but he does not say whether his

pendulum or clock was one with weights. He says he regulated his clock by equal altitudes, and then by meridional passages of Spica Virginis, and of the Sun; but why did he leave off the method of the equal altitudes which he made use of at first, and in what manner did he observe these altitudes to determine the passage either of a fixed star or of the Sun over the meridian? It does not appear that he had a quadrant or transit instrument. I am sorry that this uncertainty about the means employed by Mr. Hirst to determine the time of the phases puts it out of my power to make use of an observation, which might otherwise have been extremely useful, had the astronomer been equally well provided with instruments as he appears to have knowledge and zeal. It is to be observed that by increasing or diminishing by $10''$ the duration observed at Madras, the question of the parallax will be decided conformably either to the observation of Rodriguez or that of the Cape.

In the same volume of the Transactions, are some observations of the same transit made at Abo and at Hernosand; the total duration was observed in both places; it may have been lengthened somewhat beyond its limits; but these observations agree at least in this point with all the others that were made in the North, viz. that being compared with the Tobolsk observation, with regard to the duration of the transit, they give above $10''$ for the horizontal parallax of the Sun.

I have likewise lately had the communication of Mr. Rumowski's observation made at Selenginsk in Siberia. I shall not expatiate upon the particulars here,

supposing that you Gentlemen have received it. The latitude of Selenginsk is $51^{\circ}46'6''$. I have settled the longitude to be $6^{\text{h}}57'50''$ from the Paris meridian. Some immersions of the first and second satellite of Jupiter have given me $6^{\text{h}}57'15''$, $6^{\text{h}}57'20''$, and $6^{\text{h}}58'31''$, by comparing the observations of Mr. Rumowski with the tables corrected upon the observations made at Paris and at the Cape, and estimating, as well as I was able, the different effects of the reflectors and telescopes. The 15th of July, Mr. Le Monnier observed the meridional transit of ϕ Sagittarii at $18^{\text{h}}25'38'' \frac{2}{3}$ of his clock, being at $76^{\circ}00'45''$ from the zenith. The preceding limb of the Moon passed at $18^{\text{h}}38'28'' \frac{1}{2}$, or at $11^{\text{h}}01'40''$ apparent time; when the center passed, the distance of the upper limb was at $75^{\circ}52'00''$ from the zenith, and the lower at $76^{\circ}22'25''$; the threads, which are $5''$ thick, being entirely upon the Moon. σ Sagittarii had passed at $18^{\text{h}}35'22'' \frac{1}{2}$, the distance from the zenith being $75^{\circ}22'20''$. I have observed at Rodriguez the immersion of σ Sagittarii at $14^{\text{h}}01'28'' \frac{1}{2}$ apparent time, and Mr. Rumowski observed at Selenginsk the immersion of ϕ at $11^{\text{h}}24'51''$ apparent time. Upon comparing all these things together, I find the longitude of Selenginsk to be $6^{\text{h}}57'21'' \frac{1}{2}$ East from Paris. But this goes upon the supposition that the error of the tables has been quite constant during near 7 hours, which cannot be warranted. Lastly, the observation of the eclipse of the Sun made at Selenginsk June 3, 1761, compared with the same observation made at Tobolsk and at Cajanebourg, determines the longitude of Selenginsk $5^{\text{h}}16'41'' \frac{1}{2}$ East from Cajanebourg, and at $2^{\text{h}}34'30''$ East from Tobolsk, consequently $6^{\text{h}}58'22''$ East

East from Paris. These are the reasons which make me believe that the longitude of Selenginsk cannot be made less than $6^h 57' 50''$ from the meridian of our Royal Observatory. This being supposed, the observation of Mr. Rumowski compared with mine would give $10'' 1$ for the horizontal parallax of the Sun in the month of June, and $10'' 26$ for that parallax when the Sun is at the mean distance. 'Tis true, the same observation, compared with that of Mess. Mason and Dixon, would much reduce this parallax; and therefore it can be of no use to decide the question, unless we had some from Africa or from the adjacent seas.

I have one, which I have not dared yet to lay before our Academy; it appears to me a downright phenomenon. As it was made at the isle of France or Mauritius, it might pretend to the glory of deciding between Mess. Mason and Dixon and me; but it is such a one as can only at most determine how far the difference of sights or that of telescopes could extend or shorten the duration of the exit. I am acquainted with the observer, Mr. de Seligny, an officer in the service of the East India company; and I think I can answer for his capacity, talents, zeal and accuracy. He had no instrument but an eight foot telescope, and an excellent clock: this he regulated by altitudes of the Sun taken the 5th and 6th of June with Hadley's quadrant. That method is not strictly true, but our observer could do no better; and besides, whether the clock went too fast by a few seconds more or less, the difference cannot at all affect the duration observed by Mr. de Seligny. His clock on the 5th of June about 3 o'clock advanced $18'' \frac{1}{2}$ upon the

the apparent ; the 6th at the same hour it got only 9''. The observer was obstructed by the clouds till near the instant of the exit of Venus. This is his observation of the exit ; the times are those of his clock, from which 2'10'' are to be subtracted to have the apparent time. At 0^h28'38'' the preceding limb of Venus appeared to touch exactly that of the Sun ; at 0^h22'00'' one quarter of her diameter was got out ; at 0^h25'10'' Mr. Seligny judged her to be half got out ; at 0^h28'06'' the three quarters of her diameter were emerged, and 0^h31'12'' the exit was total and instantaneous. These observations follow one another very well, and they have been made by a man whom I know to be very skilful ; but how the exit of Venus could appear to him to have taken up but 12'34'' I must leave to persons skilled in natural philosophy to account for. The observation of the first contact, or of the internal contact of the limbs, would give you more than 12'' for the parallax ; that of the last would reduce it to eight ; by a medium, the parallax would exceed 10''.

You will undoubtedly have observed a pretty remarkable difference between my observations of Venus, as I had the honour to send them to you from Lisbon March 6, 1762, and the same observations as I have related them in my memoir. I imagined I had sent the correction to some one of the members of your celebrated Society. When I made the reduction of these observations at Rodriguez, I found myself under disagreeable circumstances ; and it was probably my uneasiness at that time that occasioned a want of due attention, which I was the longer in finding out as I could not easily sus-
pect

pect it. My clock went too slow that day at noon by 2'2'', so that I ought to have added 1'2'' to the time of the clock, whereas I subtracted as much from it. This is the reason of the difference in the times. The small variation in the distances of the limbs was owing to a stricter verification of the parts of my micrometer.

I am, with the most respectful esteem,

Paris, Feb. 14, 1764.

Gentlemen,

your most obedient,

humble servant,

Pingré.